be marked "right," but a W.N.W. wind "wrong," because this type of weather accompanying the two is different. So with regard to easterly winds, if, say N. is predicted and E.N.E. follows, this would be "right," but an E.S.E. following on the prediction of N.E. would be "wrong."

With regard to strength, the forecasts often make anything less than a gale a possibility.

The temperature forecasts are generally vague, but if a rise of temperature occurs when the prediction is "cool," this would be marked "wrong," or, again, if moderate temperature is predicted and the true temperature differs much from the average for the time of year. So far as possible I have given a favourable interpretation to all the forecasts with the results which are here shown in Table I.

TABLE I.—*Comparison of Weather in London in* 1913 *with the Forecasts made on the Previous Day. The actual Weather is Judged from the Morning Weather Chart of the Day.*

—	Wind				Temperature			
	Right	Wrong	?	No forecast	Right	Wrong	Doubtful	No forecast
January ...	18	9	—	4	15	10	2	4
February..	13	11	—	4	14	8	2	4
March ...	14	7	4	6	13	7	4	6
April ...	15	10	1	4	13	9	4	4
May ...	13	11	3	4	12	13	2	4
June ...	17	6	2	5	14	9	2	5
July ...	20	6	1	4	12	11	4	4
August ...	14	8	4	5	15	9	2	5
September	12	12	2	4	13	8	5	4
October...	13	13	1	4	15	10	2	4
November	13	9	1	7	11	7	5	7
December	19	5	3	4	14	8	4	4
Total ...	181	107	22	55	161	109	38	55
Percentage on No. of forecasts	58	34·5	7·5	—	52	36	12	—

So far therefore as the present very limited examination goes, the probability of finding the predicted weather on the morning of the day succeeding the forecast is as follows:—

	Wind	Temperature
Probability for	0·58 ...	0·52
,, against ...	0·35 ...	0·36
,, doubtful ...	0·07 ...	0·12

TABLE II.—*Change of Type of Weather.*

—	Changes of type	Forecast		Percentage of correct forecasts
		Right	Wrong	
January... ...	15	11	4	73
February ...	10	8	2	80
March	9	4	5	44
April	11	7	4	63
May	14	10	4	71
June	6	4	2	66
July	12	10	2	83
August	9	4	5	44
September ...	13	7	6	54
October ...	18	15	3	83
November ...	12	6	6	50
December .	9	7	2	78
Total	138	93	45	789
Percentage .	—	67·4	32·6	—

Table II. shows the number of changes of type of weather, as reckoned by the direction of the wind. This is intended to indicate the correctness of the forecast as regards *change* of weather, and here the percentage of success is higher, being 67·5.

It will be noticed that during the year the type of weather changed 138 times. If, therefore, anyone with no information whatever had been content with saying "To-morrow will be like to-day," he would have been wrong 138 times, and right 227 times, and the probability in favour of his prediction being 0·62, and against 0·38. With the help of a barometer and ordinary local observation, he might probably improve on this somewhat.

Comparing these figures with the results obtained by such a fairly complete knowledge of the simultaneous conditions in surrounding regions as is afforded by telegraph day by day to the Meteorological Office, it does not appear that for this latitude and country the odds in favour of the latter are large.

On looking over the immense mass of figures—considerably more than 1000 entries—which are used in the construction of each Weather Chart, I am reminded of a sentence in the late Sir G. (then Mr.) Airy's report to the board of visitors of the Greenwich Observatory in 1867, when speaking of the proposed increase in the number of meteorological observatories, "whether the effect of this movement will be that millions of useless observations will be added to the millions that already exist, or whether something may be expected to result which will lead to meteorological theory, I cannot hazard a conjecture." De Morgan quotes this sentence (the last in the report in question) in his "Budget of Paradoxes," and remarks:—"This *is* a conjecture, and a very obvious one: if Mr. Airy would have given 2½*d.* for the chance of a meteorological theory formed by masses of observations, he would never have said what I have quoted."

Personally, I think it extremely improbable that any trustworthy weather forecasts for periods so long as twenty-four hours will, or can, ever be made for regions outside latitudes 30 N. or S. or thereabouts, with the exception perhaps of a few places where the local conditions are paramount.

The reasons for this view will be given in a subsequent note. A. MALLOCK.

February 17.

The Darwinian Theory of Atolls.

IN the review of "Letters and Recollections of Alexander Agassiz" (NATURE, January 29)—an article in which we are given a picture of an exceptionally interesting personality drawn by an intimate friend—there is a brief paragraph on the formation of coral reefs to which I should wish to refer.

My friend, Sir Ray Lankester, concludes that Agassiz "certainly succeeded in showing that the views advocated by Darwin and by Dana are not capable of universal application, nor, indeed, of general validity" (pp. 603–4).

It would be unreasonable to claim "universal application," but "general validity" is a matter to be determined by a comparison of the number and extent of the regions in which Darwin's explanation holds good with the number and extent of those in which it does not. The results of the Funafuti boring offer, I think, important evidence in regard to the Pacific area. A test locality was chosen with the utmost care by unbiassed authorities. A bore-hole was drilled, at great expense and in spite of many difficulties, to a depth of 1100 ft. *Only shallow water organisms were found in the core.* For some reason or other—probably because it is more exciting to overturn than to confirm—very little has been said about this evidence. We all hope for more borings; but in the meantime the only important trial that has been made

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entirely supports the validity of Darwin's theory, in the locality selected as a test.

EDWARD B. POULTON.

Oxford, February 20.

The Accuracy of the Principal Triangulation of the United Kingdom.

THERE was some discussion of this question at the last meeting of the British Association, and an investigation by Capt. H. St. G. L. Winterbotham has been published by the Ordnance Survey (Professional Papers, new series, No. 2). It appears, however, that the measurement of Lossiemouth Base, valuable as it was, did not definitely decide the question, but that a moderate amount of further computation would do so.

(1) It will be generally admitted that statistical evidence as to the precision of any kind of observation is of no great value unless it is based on a large population. For example, if we may assume the principal triangulation to have been all executed by similar observers under similar conditions, and no unjustifiable rejections to have been made, we may accept with great confidence the probable error of ±1·23″ computed from the closure of 552 triangles. But nowadays one would not think of estimating the precision of a base-line by the discrepancy between two measurements of it, though one might reject the measurements if they did not agree as well as was to be expected from the known usual probable error of a measurement. Thus discussion of a large population of discrepancies gives a good estimate of the probable accidental error of a measurement, but a small population only fixes a lower limit to that probable error. Now we have only a population of three independent discrepancies between the four bases of Lough Foyle, Salisbury Plain, Lossiemouth, and Paris, and even if it is brought up to six by the inclusion of the three bases measured with steel chains, yet it must be considered a very small population upon which to base any estimate of precision.

(2) But the original question, to what extent the strength of the figure compensates for the large probable error of an angle, is not necessarily a question for experiment, but it is essentially a question for computation. Given the probable error of an angle in a block of triangulation adjusted rigorously by least squares, if it is required to find the probable error of the distance from any point of it to any other point, the first step is to express the unknown error in that length as a linear function of the unknown errors in a number of independent angles (*i.e.* two angles of each triangle in a chain of independent triangles stretching from one point to the other, and in another chain stretching from one of the points to the nearest base). It then remains only to add another column to the least square computation by the method described in Wright and Hayford's "Adjustment of Observations," paragraph 123. May I venture to suggest that if two or three such cases could be worked out for strong figures in the United Kingdom, and for comparison two or three cases for weaker figures, either there or elsewhere, it would not only set at rest the immediate question, but would also establish results of great importance for surveyors in general.

T. L. BENNETT.

Computation Office, Egyptian Survey Department.

ONE can but agree with Mr. Bennett in insisting on a large "population" of discrepancies upon which to found a calculation of a probable error, whether it be of the measurement of a base-line or of an angle.

His example of a base-line measurement is not, however, strictly comparable with the investigation in question. In the former a large number of independent measurements must be made from which to deduce the most probable length and the individual discrepancies from this length. In the latter the measured bases may be regarded as errorless compared with the triangulation, and the actual errors can therefore be deduced with surety.

Neglecting the steel chain bases, we have four independent measures upon any one of which the triangulation can be made to depend. These four are widely distributed.

The longest line from base to base is that from the new base at Lossiemouth to the Paris base, and along this line the old steel chain bases add additional proof that no serious errors are inherent in the triangulation.

I agree that, "if we may assume the principal triangulation to have been all executed by similar observers under similar conditions and no unjustifiable rejections to have been made, we may accept with confidence the probable error of 1·23″ computed from the closure of 552 triangles," but it must be remembered that this is the probable error of an observed angle. The date of the work makes this a matter of no surprise. The point at issue, however, is not the probable error of an observed angle, but the probable error of an adjusted angle, or, in other words, to find out how far the intricacy of the figure has compensated for the lack of precision of angular measurement.

To say that this question is essentially one for computation is not, to my mind, correct.

The probable error of the ratio of any two sides as derived from the triangulation depends upon the probable error of an adjusted angle. This calculation is possible, but from the complexity of the figure and the intricate system of weighting the angles, impracticable. Moreover, the answer to it would still be the probable, and not the actual, error.

It is, however, possible to pick out of the general figure chains of simple triangles connecting the bases. Supposing that these chains had been the only paths of calculation, and that they had shown the same errors of ratio between the bases as are actually found, we can deduce what the probable error of an observed angle would have been to have effected this result. Such an investigation has been made, and the probable error of an observed angle in these "equivalent" simple chains is 0·85″, or approximately the same as that given by General Ferrero, in his 1892 report, as the mean figure for the probable error of an observed angle in the triangulations of those twenty countries represented on the International Geodetic Association.

Although, therefore, further investigation on the lines advocated by Mr. Bennett would be one of the greatest interest, I do not think that it promises a result commensurate with the time and expense it would entail.

H. S. L. WINTERBOTHAM.

Atomic Models

I AM indebted to Mr. Chalmers for pointing out (NATURE, February 19, p. 687), what I had indeed suspected, that the magnetic moment due to an electron moving in a circular orbit, assuming the angular momentum to be $h/2\pi$, is *exactly* five times the magnetic moment of the magneton. The original value ($15{\cdot}94 \times 10^{-22}$) of the latter quantity, given by Weiss, and quoted in my former letter, was based on the value of Avogadro's constant found by Perrin. If we divide the magnetic moment of the atom gram, 1123·5, by the more recent value for Avogadro's constant given by Millikan ($60{\cdot}62 \times 10^{22}$) we obtain as the magnetic moment of the magneton $18{\cdot}54 \times 10^{-22}$, which is exactly

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